April 30, 1953

Dr. P. R. Edwards
Box 185
Chamblee, Ga.

Dear Phil:

You can probab. keep the draft unless you want to mark your revise directly on it, I should have thought to have another typed.

Enclosed is my draft for our manuscript. May I suggest the following procedure? If you will indicate your revisions or redrafting, I will have our final ms. mimeographed here. This will give enough copies to meet whatever approval requirements you may have. In addition, I want to send one each to Spicer, Stocker and Zinder. In all this, I do not mean to preclude detailed discussion: there are some parts of my draft you may not be entirely sympathatic with. I felt we could not entirely ignore the nomenclatural question, if only to emphasize a neutral attitude.

There are two points to which I would call your special attention: 1) would you make up Table 5— the absorption experiments? The protocols seem complete enough, but you might have something in mind tombe emphasized. IX XII 1:— can be called SW-570. 2) I do not know how much additional detail, if any, would be needed in the characterization of some of the cultures, for which accession numbers have been given. The main point to identifying the cultures by number is, I suppose, to ensure a permanent record of just what was used.

I wish I could remember whether I saw an Edison-Voicewriter dictating machine in your office. I do not often use the one we have in the department, but there are times when I would have a distinct preference for talking, especially on unimportant matters which did not require a duplicate record. If this is your machine, I may mail you a disc sometime in lieu of a letter. I met E. S. Anderson at Urbana last week, and mentioned Bailey's difficulties in demonstrating the determinant phages of typhi. He evidently finds it no easy task himself: the plaques are some 20 - 50 microns (sic),and need special care. He has an ms. giving technical details, and if he does not get a chance to visit Chamblee (which is doubtful, not that he would not like to now, but his itinerary is very crowded) he will send this on to you.

I am also returning a reprint request card. Absolutely unnecessary! We just haven't got these in as yet.

It looks as if our correspondence is fairly up to date now. Under separate cover, I am sending my own isolates of SW-684 ph2 (gp:1,2) which does not react with my gm serum. In addition, for the record, SW-930 (table 1). If there any other cultures in the paper that have to be rechecked, in your opinion, please let me know. I am also sending you SW-950B. This is ph2 of TM2 (a biochemical mutant derivative) and has been reacting 1,2,i quite consistently: I doubt that it is a matter of mixture of cells. As this was the recipient strain, it probably has something to do with the 1,2,gp finding. TM2 itself has shewn what may turn out to be three phases: i; 1,2; and i,1,2. The peculiarity of SW-986, abortus-equi —x TM2 follows the same pattern. I have some microscopic experts.

under way that may help to settle the story. Shades of Archer! [I now see, from my missive of 4/18 that I've been over this before.]

The b:i story gets more complicated. I'll burden you with the ~~dectile~~ details when they begin to ~~subd-~~ subside. For no particular reason, I'll send SW-1031 which looks to be a:b from sendai a —x SW 1026 i:b. The i:b types are a regular result of TM—x "N97b--SW1007", but TM--x N25b seems to give only i:—.

Again for the record. SW-1005 and SW-1036, z33:enx and z33:1,2 from S. abony (#103) amd S. paratyphi B (#3) in homologous serums. The previous isolates of this type were in transduction experiments, and I did them over again to rule out the unlikely possibility that phage or tranduction had anything to do with. I agree that z33 is a changed b, and do not regard ~~β~~ b--z_{33} as having anything to do with phase variation. I wonder of this change is any less frequent in diphasic stocks. One has rather less occasion to put diphasics into the two serums, and one always runs the risk of interfering cross-reactions. [Am having trouble with another one: Colindale's 1,5 serum vs. b kept me from getting ~~b-phases~~ b:1,5 from abony—x miami. When your 5 was used instead, it worked beautifully.]

To answer some queries in your recent letters: SW960B, etc. are of no special importance. SW-977 is very likely a j phase, from the d of ~~β~~ zega d:z6. My hunch is that it will turn out to be j:1,2. The other possibility is that it is an artificial derivative of z6. You said you were going to make some j serum sometime anyhow. Was this included in the previous tests? SW-1023 will probably turn out the same way. I have not been able to transduce z6 to miami (even with d, a, 1,2 serum) probably because of cross-reactions, and the appearance of this artificial phase.

In your question on the segregation of phage resistance: the markers used in the linkage studies were resistance to T1, T5 etc. These resistant variants were not lysogenic. Later, with lambda we expected just what you said, but in fact crosses of lysogenic x sensitive have shown a ~~shap-~~ clearcut segregation. There evidently is not much active phage under these conditions. ~~Lysoeg~~ Lysogenicity is evidently the potentiality for producing a "mature" phage, rather than its presence in the cell. For this reason, the phagologists have been talking about "prophages" as the what-is-it that is propagated in a lysogenic bacterium. When the prophage matures to phage, the well is lysed and free phage is released. Our work would indicate, without absolutely proving, that the prophage is stuck to the nucleus.

Thanks very much again ~~fre~~ for the last batch of cultures and serums. I am afraid the maltose-positive element of S. pullorum 2479-50 was a coccal contaminant. [I'd sooner get your raw material than put you to the trouble of weeding out such lemons yrself.] I've written to Hinshaw for some of his old material (on the suspicion they might turn out like SW-970).

I hope you do not necessarily put every culture I send through your diagnostic mill. ~~It'd~~ It's easier for me to include a culture that may be of possible interest than to ask you first. But perhaps it's easier and safer to handle everything this way. I will try to be reasonably explicit about what I may be trying to find out or have verified.

Sincerely,

Joshua Lederberg